PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Addressing the psychological impact of COVID-19 on healthcare workers: Learning from a systematic review of early interventions for frontline disaster responders
AUTHORS	Hooper, Jasmine; Saulsman, Lisa; Hall, Tammy; Waters, Flavie

VERSION 1 – REVIEW

REVIEWER	Oe, Misari Kurume University School of Medicine, Department of Neuropsychiatry
REVIEW RETURNED	17-Sep-2020

GENERAL COMMENTS	This manuscript focuses on psychological intervention programs for frontline workers. The results will be interesting to readers worldwide if the authors can appropriately show the results. However, I have several comments. All are major points to be considered. 1. I understand the basic concept of this systematic review, however, I do not agree with the title of this manuscript. There is no enough evidence yet that the psychological interventions shown here can protect healthcare workers against the psychological impact of COVID-19. The readers may be misled by the title. I recommend that the authors avoid overstatement. 2. It is natural to think that the frontline workers had psychological burdens before participating in an intervention program. In this sense, it is almost impossible to distinguish “treating” psychological interventions from “preventive” interventions. Therefore, I believe that interventions like trauma-focused CBT should not be excluded only because symptom reductions were described. (The authors included the study of Jarero and Uribe (2012), whose participants had posttraumatic stress symptoms. I think that this study should be excluded if one follows the rules by the authors. By the way, the study of Jarero and Uribe (2012) was severely biased and not RCT. Assignment of the treatment group was not randomized.) The authors can think of another option that including the programs planned for the frontline workers only (i.e. exclude interventions for victims). 3. It is obvious that high-school students are not frontline workers. Therefore, there seems no reason why the study of Farchi et al. was included. I think that the participants were simply “victims of witnessing unexpected car crash” and not “frontline workers”.

	4. It seems better to indicate the definition of frontline responders and psychological intervention programs in the Methodology section.

REVIEWER	Stensland, Synne Norwegian Center for Violence and Traumatic Stress Studies
REVIEW RETURNED	30-Sep-2020

GENERAL COMMENTS	Protecting healthcare workers against the psychological impact of COVID-19: A systematic review of interventions for frontline responders Dear editor, thank you for letting me review this paper, and dear authors, thanks for doing this important work. Identification of evidence based psychosocial interventions that may help mitigate adverse health outcomes among hcp during the prolonged crisis is paramount. Overall, the Objectives of the current paper; to i) conduct a systematic review of psychological interventions administered to frontline responders exposed to mass trauma or major disasters, and ii) further discuss suitability of implementing such programs within the healthcare workforce during the covid-19 pandemic is relevant and results may potentially be of high utility. It may be useful to clarify whether the systematic review was set to include both individual, social/collegial and organizational/structural interventions. The sources; Embase, Web of Science, PsycINFO, and Google Scholar, for search of relevant published studies are suitable. Systematic reviews are not my main area of expertise, yet I wonder why Pubmed; as one of the major medical search engines was not included. Outcome measures including psychological functioning outcomes of distress and positive change are relevant, as are intervention effectiveness, content applicability, and feasibility. It could we helpful to exemplify the term 'psychological functioning outcomes of distress' in the abstract; are we talking about ability to work, days of sick-leaves, fatigue or other functional measures? Following are more specific remarks. The study flow diagram: The flow-chart is a little confusing as excluded articles/titles are both included within the main stem and in boxes leading out of the main stem. Boxes g excluded articles/material have their arrow pointing towards the stem. Often such arrows point in the opposite direction to emphasize that this material is taken out/excluded. The flow-chart needs to adhere to BMJ Open standards. Introduction: Burn-out and/or fatigue are common, relevant outcomes in response to the prolonged crisis that have been studied among health care personnel prior to the pandemic. These should be included in the introduction considered the aim 'include psychological functioning outcomes of distress'. Inclusion of this perspective helps accommodate the long-term aspect of the current crisis. The exposure, tasks and efforts taken on by health and social care workers such as nurses, doctors, paramedics, and forensic workers as well as other security personnel such as police officers and the military may differ vastly depending upon contextual measures. It seems to be hard to claim that they 'share similar experiences of trauma'. Such a statement would need some

	contextualization and at least a reference. Eventually the reader needs to understand why you are including a range of different personnel. Further, in the second paragraph it seems like 'burnout' is handled as a stressor rather than an adverse outcome of continuous distress. This may not be the intention? Another prominent stressor to be included relates to the elevated workload related to shortage of staff, due to chronic shortage within the services in combination with the current pandemic related shortage due to quarantine, sick-leaves, personnel staying at home to care for kids out of school etc. The third paragraph starts up with mentioning coverage of basic needs related to protection from contagion of the virus. These may be seen as interventions of an organizational art. Following, authors refer to personal help-lines etc. Such individual interventions requiring active help seeking behavior from psychologically distressed health care personnel (hcp) have in previous systematic reviews been found to be of and in little use. A differentiation and introduction to levels of interventions (organization etc) would be helpful. Methods: The title of the article is 'Protecting healthcare workers against the psychological impact of COVID-19: A systematic review of interventions for frontline responders' – yet, in the first paragraph of the methodology section it says 'defined here as individuals trained to provide services in emergency or disaster settings, such as healthcare workers or security forces;' I would suggest sticking to hcp. This would adhere to your choice of search words. Eventually, the reader needs to understand why you are including a range of different personnel. Regarding sources for the systematic review I wonder why PubMed was not included. Regarding searchwords I wonder why terms commonly used as potential measures of level of distress/adverse functional outcomes among hcp such as fatigue and burnout were not included? Results Under the paragraph about Eye movement desensitization and reprocessing (EMDR) the authors write that 'Given that healthcare professionals share similar workplace experiences to other frontline staff, EMDR appears a very applicable intervention for reducing PTSD rates in this population.' If there are studies supporting this statement it should be stated. If not, the sentence is too 'convinced', and should be rewritten to encompass doubt. Also, EMDR treatment is costly in most regions. This must be stated. Under the paragraph on Resilience and coping for the healthcare community (RCHC) Effectiveness the authors state that: 'The RCHC uses a risk and resilience framework that has been carefully adapted for use with healthcare and social service providers. Therefore, this intervention is very suitable for the healthcare workforce.' Such statements need some descriptives and a reference. Discussion Based on this systematic review, where you have found that a number of the interventions described seem to be somewhat effective – given the sparse number of studies – what would be the authors basis for recommending only the two - PFA and EMDR (ie. two studies)? I suggest presenting the different alternatives, uncertainty (lack of evidence) and base discussion on comparison

	to findings from therapeutic interventions targeting other comparable trauma-exposed populations. A limitation that needs to be elaborated and added early on in the paper is the ongoing character of the covid-19 pandemic, putting a high level of stress on hcp over time. This may in part deviate from the acute trauma of an accident and similar traumatic events.

REVIEWER	Zhang, Tao Fudan University
REVIEW RETURNED	07-Oct-2020

GENERAL COMMENTS	This paper conducted a systematic review of psychological interventions administered to frontline responders exposed to mass trauma or major disasters and discussed the suitability of implementing these programs within the healthcare workforce. The topic is interesting, while I have some concerns about the manuscript. Why the authors only search these three databases? Are these databases could cover the most psychological studies? It's unclear for me how the author could draw the conclusion PFA and EMDR are the most suitable interventions? It's hard for readers to get the point. In the results section, the author did not provide the practicable information about the interventions, for example ICF-PFA. They should try to get detail information first.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name
Misari Oe

Institution and Country
Kurume University, Japan

Please state any competing interests or state 'None declared':
None declared

Comments to the Author
This manuscript focuses on psychological intervention programs for frontline workers. The results will be interesting to readers worldwide if the authors can appropriately show the results. However, I have several comments. All are major points to be considered.

1. I understand the basic concept of this systematic review, however, I do not agree with the title of this manuscript. There is no enough evidence yet that the psychological interventions shown here can protect healthcare workers against the psychological impact of COVID-19. The readers may be misled by the title. I recommend that the authors avoid overstatement.
Thank you for your overall review on the paper and for this suggestion. In order to address this comment, we have changed the title to "Addressing the psychological impact of COVID-19 on healthcare workers: Learning from a systematic review of early interventions for

frontline disaster responders".

2. It is natural to think that the frontline workers had psychological burdens before participating in an intervention program. In this sense, it is almost impossible to distinguish "treating" psychological interventions from "preventive" interventions. Therefore, I believe that interventions like trauma-focused CBT should not be excluded only because symptom reductions were described.
Thank you for this suggestion. We agree that symptom reduction should be included as a desired outcome of the interventions, as this seems inevitable for a frequently trauma exposed population. To align with this comment, we have changed the definition of 'preventive interventions' to 'early psychological interventions' throughout the paper, which includes both the prevention and reduction of mental health symptoms. Therefore, we now describe early psychological intervention (on page 6) as "programs designed to prevent or reduce mental health issues from trauma exposure, through increasing positive mental health outcomes such as resilience, coping, and life satisfaction and/or reducing negative mental health outcomes such as PTSD, depression, and anxiety." As a result of this change, we expanded the eligibility criteria on page 7, for example "Early psychological interventions designed to prevnt the development of mental health issues at pre, during, or post-disaster stages or reduce mental health issues with delivery commencing within three months of exposure to a traumatic event". Based on this definition, we removed the exclusion of trauma-focused CBT in the paper. However, we identified no papers that evaluated trauma-focused CBT in frontline workers. We also recommended trauma-focused CBT as an option for people with severe or persistent symptoms needing long-term individualized treatment (see discussion section on page 21), "Nevertheless, it is still recommended that anyone with severe or persistent trauma-related symptoms should seek out more intensive and longer-term individualized support, such as trauma-focused CBT." Based on this updated definition, we removed ICF-PFA from the included and reviewed studies and added RAW, which we believe now suits this eligibility criteria. See added sections on the RAW program throughout the paper, in particular pages 10, 12, 13, 14, 16, and 17.
(The authors included the study of Jarero and Uribe (2012), whose participants had posttraumatic stress symptoms. I think that this study should be excluded if one follows the rules by the authors. By the way, the study of Jarero and Uribe (2012) was severely biased and not RCT. Assignment of the treatment group was not randomized.) The authors can think of another option that includes the programs planned for the frontline workers only (i.e. exclude interventions for victims).
Thank you for your suggestion regarding the Jarero and Uribe (2012) paper. Now that we have modified the definition of included interventions to 'early psychological interventions' that seek to prevent or reduce mental health impact, rather than just 'preventive' interventions, the Jarero and Uribe (2012) paper should fit the inclusion criteria. Thank you for also pointing out the error in Table 1 of my paper regarding the study design of the Jarero and Uribe (2012). Indeed, their study used a quasi-experimental pre/post treatment 'field study' design rather than a RCT. We have chosen to keep the study, as RCTs are very difficult and sometimes impossible to conduct due to the nature of traumatic stress environments during disasters. Ethically, we believe that inclusion of non-randomized studies reduces study selection bias and reflects more real-world results than if studies were only included with fairly controlled 'safe' environments. We have edited Table 1 to reflect the corrected error, see page 9 "Quasi-experimental design", "immediate group", and "for both the immediate and waitlist/delayed treatment groups". We have also edited the summary of study characteristics on page 11 to reflect "five" RCTs and "three" quasi-experimental designs. Finally, we have edited the discussion on page 19 to reflect EMDR and PFA showing improvements across at least two "studies" each rather than "RCTs".
Thank you for your suggestion regarding excluding interventions for victims. We believe that the new definition of 'early psychological intervention' addresses this issue as it includes symptom reduction as well as symptom prevention. Excluding interventions designed for community trauma victims would narrow the scope of the paper and would miss important interventions (such as PFA) that may be useful for healthcare and other frontline worker populations. Therefore, we have chosen to keep interventions that target trauma victims, as long as they have also been tested in frontline workers.

3. It is obvious that high-school students are not frontline workers. Therefore, there seems no reason why the study of Farchi et al. was included. I think that the participants were simply “victims of witnessing unexpected car crash” and not “frontline workers”.
Thank you for your comment. We agree with this statement and, upon reflection, we have decided to remove the Farchi et al (2018) paper from the included interventions. Consequently, we have added a statement in the risk of bias section on page 8, “was therefore excluded” and have removed ICF-PFA from the results section of the paper. Instead, we have created a new paragraph in the discussion regarding promising interventions that did not meet inclusion criteria for this paper but deserve mention and need more research (see page 21).

4. It seems better to indicate the definition of frontline responders and psychological intervention programs in the Methodology section.
Thank you for your suggestion. We agree that definition of frontline responders and psychological intervention programs should be included in the methodology section and have added such definitions on page 6, “ Frontline workers are defined here as individuals trained to provide services in emergency or disaster settings, such as healthcare workers and security forces. Early psychological interventions are described here as programs designed to prevent or reduce mental health issues from trauma exposure, through increasing positive mental health outcomes such as resilience, coping, and life satisfaction and/or reducing negative mental health outcomes such as PTSD, depression, and anxiety. Psychological programs may involve person-directed interventions using individual or group format and structural interventions that encourage improved mental health response of the whole organization.”
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Reviewer: 2
Reviewer Name
Synne Øien Stensland

Institution and Country
Norwegian Centre for Violence and traumatic stress studies

Please state any competing interests or state ‘None declared’:
None declared

Comments to the Author
Protecting healthcare workers against the psychological impact of COVID-19: A systematic review of interventions for frontline responders

Dear editor, thank you for letting me review this paper, and dear authors, thanks for doing this important work. Identification of evidence based psychosocial interventions that may help mitigate adverse health outcomes among hcp during the prolonged crisis is paramount.

Overall, the Objectives of the current paper; to i) conduct a systematic review of psychological interventions administered to frontline responders exposed to mass trauma or major disasters, and ii) further discuss suitability of implementing such programs within the healthcare workforce during the covid-19 pandemic is relevant and results may potentially be of high utility. It may be useful to clarify whether the systematic review was set to include both individual, social/collegial and organizational/structural interventions.
Thank you for your overall review of this paper and for this suggestion. We have included a sentence in the methodology section on page 6 to reflect this clarification, “Psychological programs

may involve person-directed interventions using individual or group format and structural interventions that encourage better mental health response of the whole organization." Similarly, we have included a sentence in the summary of study characteristics section, "Studies testing EMDR and RAW programs involved individual, person-directed intervention and the single study testing the RCHC program involved a group format intervention. PFA, APD, and TRiM studies involved structural intervention across the whole organization through team training and stepped care approaches." Throughout the Early Psychological Intervention Programs section of the results (starting on page 11) and Table 2 on page 14, we have added clarification of the type of intervention for each discussed program. Finally, we have addressed this clarification issue by adding a sentence in the discussion on page 19, "comprising three person-directed interventions and three structural level interventions."

The sources; Embase, Web of Science, PsycINFO, and Google Scholar, for search of relevant published studies are suitable. Systematic reviews are not my main area of expertise, yet I wonder why Pubmed; as one of the major medical search engines was not included.
Thank you for your question. We chose not to include Embase instead of Pubmed as Medline is a subset of PubMed, overlapping by approximately 98%. Embase includes all of Medline, plus additional articles, thus includes most of PubMed (98%). Please see https://kemh.libguides.com/library/search_tips/faqs/difference_between_pubmed_medline_embase for more information.

Outcome measures including psychological functioning outcomes of distress and positive change are relevant, as are intervention effectiveness, content applicability, and feasibility. It could we helpful to exemplify the term 'psychological functioning outcomes of distress' in the abstract; are we talking about ability to work, days of sick-leaves, fatigue or other functional measures?
Thank you for your suggestion. We have addressed this concern by adding in clarification in the abstract on page 2 regarding "psychological distress (e.g. general psychopathology, PTSD, depression, stress)" and "positive mental health domains (e.g. coping, resilience, life satisfaction)". Throughout the paper, we have clarified these outcomes further. For example, we added a sentence in the introduction on page 5, "In contrst, positive mental health domains such as resilience may serve to protect the mental health of first responders", in the methodology on page 6, "Prevention programs may focus on increasing positive mental health outcomes such as resilience, coping, and life satisfaction and/or reducing negative mental health outcomes such as PTSD, depression, and anxiety.", and on page 7 under eligibility criteria, "psychological outcome measurements of positive or negative mental health outcomes".

Following are more specific remarks.
The study flow diagram: The flow-chart is a little confusing as excluded articles/titles are both included within the main stem and in boxes leading out of the main stem. Boxes g excluded articles/material have their arrow pointing towards the stem. Often such arrows point in the opposite direction to emphasize that this material is taken out/excluded. The flow-chart needs to adhere to BMJ Open standards.
Thank you for your suggestion. We have altered the flow chart to only show excluded articles in the boxes leading out of the main stem, by adding "Duplicated removed(n=51)". We have also altered arrows so that are pointing in the opposite direction, in order to adhere to BMJ Open standards. Finally, numbers have been edited to reflect the current paper edits outlined in this letter.

Introduction:
Burn-out and/or fatigue are common, relevant outcomes in response to the prolonged crisis that have been studied among health care personnel prior to the pandemic. These should be included in the introduction considered the aim 'include psychological functioning outcomes of distress'. Inclusion of this perspective helps accommodate the long-term aspect of the current crisis.

Thank you for your suggestion. We agree that burnout, compassion fatigue, and secondary traumatization should be mentioned as relevant psychological distress outcomes. Therefore, we have added a sentence in the introduction at the beginning of page 4 to reflect this comment, “as well as burnout, compassion fatigue, and secondary traumatization” and at the end of page 4, “such as burnout and compassion fatigue”.

The exposure, tasks and efforts taken on by health and social care workers such as nurses, doctors, paramedics, and forensic workers as well as other security personnel such as police officers and the military may differ vastly depending upon contextual measures. It seems to be hard to claim that they ‘share similar experiences of trauma’. Such a statement would need some contextualization and at least a reference. Eventually the reader needs to understand why you are including a range of different personnel.
Thank you for your suggestion. We have reworded this statement in the introduction on page 5 to acknowledge both differences and similarities between frontline workers and add references, “Despite obvious differences in job demands across various frontline services, these workers all face frequent trauma exposure at work”.

Further, in the second paragraph it seems like ‘burnout’ is handled as a stressor rather than an adverse outcome of continuous distress. This may not be the intention? Another prominent stressor to be included relates to the elevated workload related to shortage of staff, due to chronic shortage within the services in combination with the current pandemic related shortage due to quarantine, sick-leaves, personnel staying at home to care for kids out of school etc.
Thank you for your suggestion. We agree that burnout should not be handled as a stressor, therefore we have included it in the first paragraph of the introduction (see above comment) and have removed it from the list of stressors in the second paragraph, instead including it as a secondary effect in this paragraph. With regards to the suggestion for including additional stressors, we have included a sentence on page 4 “This demand is exacerbated by the chronic shortage of staff within frontline services and even greater shortage during the current COVID-19 climate due to quarantine, sick leave, and increased personal demands from looking after children out of school”.

The third paragraph starts up with mentioning coverage of basic needs related to protection from contagion of the virus. These may be seen as interventions of an organizational art. Following, authors refer to personal help-lines etc. Such individual interventions requiring active help seeking behavior from psychologically distressed health care personnel (hcp) have in previous systematic reviews been found to be of and in little use. A differentiation and introduction to levels of interventions (organization etc) would be helpful.
Thank you for your suggestion. We have added several sentences in this paragraph on page 4/5 to better clarify the different types of interventions available. As recommended, we added “organization-wide” to the first sentence. We also added a sentence “However, such individual interventions may be of little use as they require active help-seeking behaviour and stigma regarding mental health has been identified as a substantial barrier to seeking psychological support amongst healthcare workers”. We also felt it was necessary to recognize the extensive lists of educational information now available online for healthcare workers, so have also added the sentence “There is also a wealth of ad-hoc stress management instructions available online to healthcare workers during COVID-19, which provide lists of basic educational information about psychological self-care and help-seeking.”.

Methods:
The title of the article is ‘Protecting healthcare workers against the psychological impact of COVID-19: A systematic review of interventions for frontline responders’ – yet, in the first paragraph of the methodology section it says ‘defined here as individuals trained to provide services in emergency or disaster settings, such as healthcare workers or security forces;’ I would suggest sticking to

hcp. This would adhere to your choice of search words. Eventually, the reader needs to understand why you are including a range of different personnel.
Thank you for your suggestion. We chose to widen our search to all frontline disaster workers, including healthcare workers and security forces, due to the limited evidence-base with healthcare workers alone. We believe that research on frontline workers can inform the healthcare worker literature, given that these workplaces involve exposure to traumatic events. We have included several sentences throughout the paper to outline our reasoning for including all frontline workers more clearly. For example on page 5 in the introduction, “drawing on research from various frontline workforces” and on page 21 in the discussion, “Since the evidence-base for early psychological interventions specifically within healthcare workers is limited and other frontline personnel face exposure to traumatic events in the workplace, all disaster responders were considered in this review.”

Regarding sources for the systematic review I wonder why PubMed was not included.
Thank you for your question. We chose not to include Embase instead of Pubmed as Medline is a subset of PubMed, overlapping by approximately 98%. Embase includes all of Medline, plus additional articles, thus includes most of PubMed (98%). Please see https://kemh.libguides.com/library/search_tips/faqs/difference_between_pubmed_medline_embase for more information.
Regarding searchwords I wonder why terms commonly used as potential measures of level of distress/adverse functional outcomes among hcp such as fatigue and burnout were not included?
Thank you for your question. We agree with this statement, there are several outcome measures such as compassion fatigue, burnout, and stress that were not included in the keyword search terms. We hope that other search terms such as mental health and psychological impact would ‘hit’ articles that measures these outcomes. However, extensive searches of google scholar and reference-lists was conducted in order to account for any missed articles due to any limits of the search strategy. All psychological distress and positive mental health outcomes were considered when manually searching articles.

Results
Under the paragraph about Eye movement desensitization and reprocessing (EMDR) the authors write that ‘Given that healthcare professionals share similar workplace experiences to other frontline staff, EMDR appears a very applicable intervention for reducing PTSD rates in this population.’ If there are studies supporting this statement it should be stated. If not, the sentence is too ‘convinced’, and should be rewritten to encompass doubt. Also, EMDR treatment is costly in most regions. This must be stated.
Thank you for your suggestion. We agree that this sentence is an overstatement and should encompass some doubt. We have edited this sentence on page 15/16 of the results and added a reference, “Given these findings and that disaster workers across healthcare, forensic, and first responder populations are frequently exposed to traumatic events at work and are prone to secondary traumatization,(4) EMDR may also be an applicable intervention for reducing trauma-related symptoms in healthcare providers.”

Under the paragraph on Resilience and coping for the healthcare community (RCHC) Effectiveness the authors state that: ‘The RCHC uses a risk and resilience framework that has been carefully adapted for use with healthcare and social service providers. Therefore, this intervention is very suitable for the healthcare workforce.’ Such statements need some descriptives and a reference.
Thank you for your suggestion. We have added a reference, expanded, and edited this sentence on page 17 as recommended, “The RCHC uses a risk and resilience framework that has been carefully adapted for use with healthcare and social service providers by acknowledging the high-risk exposure of this workforce and the incorporation of appropriate strategies to build resilience.(48) Therefore, RCHC contains suitable content as it was explicitly designed for the healthcare workforce.”

Discussion
Based on this systematic review, where you have found that a number of the interventions described seem to be somewhat effective – given the sparse number of studies – what would be the authors basis for recommending only the two - PFA and EMDR (ie. two studies)? I suggest presenting the different alternatives, uncertainty (lack of evidence) and base discussion on comparison to findings from therapeutic interventions targeting other comparable trauma-exposed populations.

Thank you for your suggestion. We agree that the recommendation for PFA and EMDR as the most suitable interventions is overstated and a more objective outlook should be taken. To align with this view, we have added a sentence on page 19 of the discussion, “Generally, the evidence-base was limited across all intervention programs.” and edited a sentence “Out of the sparse number of studies, PFA and EMDR were the only programs that had been tested in frontline disaster responders across multiple studies, in addition to being applicable and feasible for rapid implementation within the healthcare workforce (see Table 2).”
We have included two more paragraphs of the discussion on page 20, in order to outline the different alternatives to the studies included in this paper and to compare findings to other trauma-exposed populations. The first paragraph added involves a list of promising interventions that did not meet inclusion criteria for this paper, but are worth mentioning nonetheless. The second paragraph addresses the suggestion to compare findings to other trauma-exposed populations.

A limitation that needs to be elaborated and added early on in the paper is the ongoing character of the covid-19 pandemic, putting a high level of stress on hcp over time. This may in part deviate from the acute trauma of an accident and similar traumatic events.
Thank you for your suggestion. We have added a sentence on page 21 of the discussion, “Additionally, the ongoing character of the COVID-19 pandemic may induce longer periods of elevated traumatic stress in frontline workers compared to the acute trauma of local disasters and accidents, differentiating this context from previous disaster events.”

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Reviewer: 3
Reviewer Name
Tao Zhang

Institution and Country
Fudan University, China

Please state any competing interests or state ‘None declared’:
None declared

Comments to the Author
This paper conducted a systematic review of psychological interventions administered to frontline responders exposed to mass trauma or major disasters and discussed the suitability of implementing these programs within the healthcare workforce. The topic is interesting, while I have some concerns about the manuscript.

Why the authors only search these three databases? Are these databases could cover the most psychological studies?
Thank you for your query. Embase, Web of Science, PsycINFO, and google scholar were searched. PsycINFO is the most popular psychology database and includes most psychological

studies, however, the other databases were included for additional scope. Medline is a subset of PubMed, overlapping by approximately 98%. Embase includes all of Medline, plus additional articles, thus includes most of PubMed (98%).

It's unclear for me how the author could draw the conclusion PFA and EMDR are the most suitable interventions? It's hard for readers to get the point.
Thank you for your suggestion. We agree that the recommendation that PFA and EMDR are the most suitable interventions is overstated and a more objective outlook should be taken. To align with this view, we have added a sentence on page 19 of the discussion, "Generally, the evidence-base was limited across all intervention programs." and edited the sentence on page 19 of the discussion, "Out of the sparse number of studies, PFA and EMDR were the only programs that had been tested in frontline disaster responders across multiple studies, in addition to being applicable and feasible for rapid implementation within the healthcare workforce (see Table 2)." Note that we have now referred to table 2, which shows that PFA and EMDR were the only programs to satisfy 'yes' to all suitability criteria.

In the results section, the author did not provide the practicable information about the interventions, for example ICF-PFA. They should try to get detail information first.

Thank you for your suggestion. We have removed ICF-PFA from the results due to the sample of participants not meeting our eligibility criteria, which should indirectly address this issue.

VERSION 2 – REVIEW

REVIEWER	Oe, Misari Kurume University School of Medicine, Department of Neuropsychiatry
REVIEW RETURNED	18-Dec-2020

GENERAL COMMENTS	The manuscript has been much improved. I appreciate the authors' great efforts that the authors have made in response to my questions and concerns.

REVIEWER	Stensland, Synne Norwegian Center for Violence and Traumatic Stress Studies
REVIEW RETURNED	05-Jan-2021

GENERAL COMMENTS	Dear editor-in-chief Adrian Aldcroft and authors of the manuscript Thank you for letting me review the manuscript Addressing the psychological impact of COVID-19 on healthcare workers: Learning from a systematic review of early interventions for frontline disaster responders. In this study authors aim to identify evidence based psychosocial interventions that may help mitigate adverse health outcomes among health care professionals (hcp) during the prolonged covid-19 crisis. Early psychosocial interventions for front-line workers are assessed. The authors' aim, methods and results are of importance to the field. Further, authors have largely done a good job reviewing their manuscript in line with comments. Yet, I miss response to review to gain a more structured approach to presenting the different interventions assessed, in line with commonly used definitions and subclassifications. It could be helpful if authors referenced the recently published third edition of the ISTSS guidelines – Effective treatments for PTSD, edited by

	Forbes, Bisson, Monson and Berliner, the Guilford Press, 2020. Using the definition of early interventions (starting within three months post trauma) and the common subclassification of such such interventiosn as used in the Guidelines mentioned above (Chapter 8, Early interventions for trauma-related psychopathology) could help structure presentation of interventions all thorugh the manuscript. The subclassifications of the presented interventions that could be helpful to use are ie. universal interventions (single or multiple), selective/indicated interventions directed towards those with an elevated symptomburden (single or multiple session) and early treatments (single or multiple session). A more specific comment relates to the use of 'multiple' studies as evidence for ie EMDR in this systematic review; as used in the abstract, tables and discussion. When referring to the evidence behind 2-3 studies I would suggest not using the word 'multiple', which may lead the reader to perceive evidence as 'overwhelming', but rather mention the actual number of studies. Last, I'd like to wish the editor and authors a Happy New Year. All the best.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Dr. Misari Oe, Kurume University School of Medicine
Comments to the Author:
The manuscript has been much improved. I appreciate the authors' great efforts that the authors have made in response to my questions and concerns.

The authors would like to thank you for your helpful suggestions and contributions to the manuscript throughout the revision process.

Reviewer: 2
Dr. Synne Stensland, Norwegian Center for Violence and Traumatic Stress Studies, Oslo University Hospital
Comments to the Author:
Review the Manuscript: bmjopen-2020-044134.R1
Journal: BMJ Open
Editor-in-chief: Adrian Aldcroft
Manuscript Title: Addressing the psychological impact of COVID-19 on healthcare workers: Learning from a systematic review of early interventions for frontline disaster responders
Due date: January 15th, 2021

Dear editor-in-chief Adrian Aldcroft and authors of the manuscript

Thank you for letting me review the manuscript Addressing the psychological impact of COVID-19 on healthcare workers: Learning from a systematic review of early interventions for frontline disaster responders.
In this study authors aim to identify evidence based psychosocial interventions that may help mitigate adverse health outcomes among health care professionals (hcp) during the prolonged covid-19 crisis. Early psychosocial interventions for front-line workers are assessed. The authors' aim, methods and results are of importance to the field. Further, authors have largely done a good job reviewing their manuscript in line with comments. Yet, I miss response to review to gain a more structured approach

to presenting the different interventions assessed, in line with commonly used definitions and subclassifications. It could be helpful if authors referenced the recently published third edition of the ISTSS guidelines – Effective treatments for PTS, edited by Forbes, Bisson, Monson and Berliner, the Guilford Press, 2020. Using the definition of early interventions (starting within three months post trauma) and the common subclassification of such such interventiosn as used in the Guidelines mentioned above (Chapter 8, Early interventions for trauma-related psychopathology) could help structure presentation of interventions all thorugh the manuscript. The subclassifications of the presented interventions that could be helpful to use are ie. universal interventions (single or multiple), selective/indicated interventions directed towards those with an elevated symptomburden (single or multiple session) and early treatments (single or multiple session).

Thank you for your suggestion and for your time and effort that has gone into providing suggestions and edits for this manuscript. We have added in a definition of early interventions according to the ISTSS guidelines, in paragraph 1 of the Methodology (page 6). Please see the new changes highlighted in blue. We have also added new classifications in the methodology on page 6, "According to the recent ISTSS guidelines, interventions were further classified as universal with single or multiple prevention sessions, selective/indicated with single or multiple prevention sessions, or early treatment with single or multiple treatment sessions. Universal interventions target all trauma-exposed individuals regardless of risk, selective/indicated interventions target individuals at risk of developing symptoms or with early signs of symptoms, and early treatment interventions target individuals after the development of a disorder."

We have mentioned these classifications in the eligibility criteria on page 7. We have also presented these classifications in Table 1 on page 9 and in the Summary of Study Characteristics on page 11. In the "Early psychological intervention programs' section we have added headings 'Universal', 'Selective/Indicated or Early Treatment', 'Universal and Selective/Indicated'. We have also added lines to table 2 on page 14 and a sentence and table to the discussion to provide a summary of recommendations according to these classifications.

A more specific comment relates to the use of 'multiple' studies as evidence for ie EMDR in this systematic review; as used in the abstract, tables and discussion. When referring to the evidence behind 2-3 studies I would suggest not using the word 'multiple', which may lead the reader to perceive evidence as 'overwhelming', but rather mention the actual number of studies.

Thank you for your suggestion. As advised, we have changed the wording to reflect the actual number of studies instead of 'multiple'. Please see changes highlighted in blue throughout the manuscript, including paragraph 3 of the Abstract on page 2, in Table 2 on page 14, and paragraph 2 of the discussion.

Last, I'd like to wish the editor and authors a Happy New Year.